A Physiological Basis for an Improved Cardiac Pathology

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A PHYSIOLOGICAL BASIS FOR AN IMPROVED CARDIAC PATHOLOGY.[1]

I.

After having been engaged for some years past in continuous investigation of the physiology of the hearts of animals, it occurred to me to inquire what new light these investigations, and those of others of a similar kind, might throw on the pathology of cardiac disease. On comparing the best articles of recent date in standard works, and also recent utterances in the form of special courses of lectures, I became satisfied that a conviction, deepened more and more during the course of my investigations, was well founded, viz.: That with our present physiological knowledge, the usual explanations of pathological conditions of the heart are too *mechanical*. This has probably to be credited to physiology, however; for with the progress made in experiment by means of complicated apparatus, we seem to have got into a mechanical way of viewing physiological processes; in other words, our thought smacks of the workshop—the laboratory. But nature refuses to be interpreted by any one rigid method. To have a just idea of her *modus operandi* in an organism we must combine all possible methods of observation, and even then humbly admit that we see but a small portion of the truth. But in asking the reader to consider what I have to present, it is but right that I should not only give him the grounds on which my opinions are based as evidence, which he can weigh for himself, but that I should candidly tell him

[1] Read in abstract at the meeting of the Canadian Medical Association, August 31, 1887.

of the paths I have been treading, and leave him then to judge of the value of the opinions advanced. Well, then, during the past three years I have studied, by the comparative method, the hearts of the water-tortoise, the sea-turtle, the alligator, the fish, menobranchus, and the snake. The safe method of direct observation with the use of simple apparatus has been employed, and the heart has in all cases been left *in situ* and kept in good condition as to nutrition, or the relation between the latter and the result has been noted. These methods are not the ones most fashionable with physiologists at the present day; they are very laborious and time-consuming, but, in my opinion, they give results which, so far as they go, are reliable; and they teach a thousand things which to the physician are fraught with instruction. One's opinions on any subject are determined not alone by what he can clearly put into words, but also by much that only the individual immediately engaged can ever appreciate; and the value of conclusions, therefore, must always depend a good deal upon the mind that forms them, as well as the methods by which they are reached. I shall, then, in this paper, endeavor to give a basis for the views I advance, at once clinical,[1] pathological, and physiological, or, in some instances, rather biological. It will be borne in mind that almost all the knowledge which I purpose to use, so far as the cardiac nerves themselves are concerned, has accrued from the investigations of the last half-dozen years, and especially the last three or four. I propose, in this paper, to attempt to substitute for mechanical explanations as applied to cardiac pathology, neuro-trophic ones; to show that what has in many cases been denominated the cause of such and such an affection, has really only been the *occasion*. It will be impossible to cover the whole ground of cardiac pathology,

[1] Unless otherwise announced, my authority for clinical and pathological statements will be the articles on cardiac diseases in the Am. System of Medicine (Pepper), in the first series of which, especially, there is a determined effort evidenced to place the views advanced upon as good a physiological basis as the knowledge then available permitted.

so I must endeavor to make the character of the explanations I would apply, clear, rather than to furnish detailed solutions for each case.

It will be found that writers on cardiac diseases have been accustomed to lay great stress on sexual excess as a causative factor in various affections, including the so-called irritable heart; and many writers[1] are of opinion that "heart-strain" is a real cause of break-down in athletes; but explanations of this result are either wholly wanting or are mostly of that mechanical kind, the validity of which I shall call in question. I have not the least doubt that these views, as to the actual connection of break-down with the factors associated with it, are correct in fact; and the explanation I would submit is to this effect: In every living organism there is a possible maximum of vital force. In organisms with a nervous system there is a certain similar maximum of available nervous energy; the amount depending on the condition of the animal at the time—just as with a Daniell's cell there is only so much electricity generated, even when in the best possible condition—which may be very much less if the conditions of the battery's action are not perfect. If a portion of this electric force be diverted along one conductor, there is so much less left for other possible channels; thus it is with the nervous system. To apply this, first, to sexual excesses and their consequent effects on the organism, the heart included.

When the sexual orgasm—well so called—is regarded physiologically, it is seen to involve the most violent discharge of nerve-force that ever takes place in a normal animal. It is, in fact, comparable to a pathological convulsion. It is physiologically justifiable only when there is a surplus of nerve-force; and then it may produce that equipoise of forces after which nature seems everywhere to be striving. But except under these conditions it is a drain upon the store of vital force incom-

[1] Am. Syst. of Med., vol. iii., p. 629 et al.

parably the greatest known to us. What happens in the oft-repeated sexual act is well illustrated by the functional action of electric fishes. The electric discharge is correlated in these animals with a corresponding nervous discharge, the physiological relations of which are now well understood. But after a certain number of discharges and in a very short space of time the fish is exhausted, and requires rest before there can be a functional repetition. Sexual excess implies that there is loss upon loss. It remains now to show how this is related to enfeeblement and disease in organs distant from the nervous system.

I propose to attempt to show that there is *continually* flowing out from the nervous centres, along the nerves, influences *essential* for the nutrition of the tissues; and that every functional act implies a corresponding nervous outflow, not only as a part of the connected chain of events for the manifestation of that function, but for the actual healthy nutrition of any tissue to which nerves are supplied. I propose to go further, and show that the functional action of a tissue (*i.e.*, an aggregation of cells) is but one of a series of changes which are closely bound up, if not identical, with the nutrition of that tissue.

The meaning of this will be clearer after the citation of a few facts—clinical, pathological, and physiological. That constant worry, grief, and the whole class of depressing emotions are associated with impaired appetite and nutrition is a matter of the commonest observation; and that the opposite class of emotions leads to contrary effects is equally well known. But what has not been at all clear is the *rationale* of this really wonderful connection.

Both pathology and physiology assist in the explanation. The most remarkable results have followed the secondary suture of nerves, accidentally divided long before. Thus in a case in the Montreal General Hospital, in which Roddick[1] sutured the sciatic nerve that

[1] Canada Med. and Surg. Journal, January, 1877.

had been divided seventeen months previously, the healing of two troublesome ulcers followed immediately. To what else could this possibly be due, if not to influences passing along nerves from centres of nerve-force —we might say, of vital force? Such cases make more clearly, perhaps, for the view I am advocating than others in which the anatomical pathological changes in the nerve-centres have been well known.[1] The dependence of paralysis on localized central nervous changes has long been recognized, though the meaning of the connection has not been hitherto clearly pointed out. The fact that in some forms of paralysis (hysterical) there is not wasting, goes far to establish the view I entertain of a nutritive influence *constantly* passing along nerves. A skeletal muscle degenerates after section of its nerves; and if the result is delayed when electricity is used, it is, I would suggest, because this agent simulates nerve-action, though imperfectly. There are physiological experiments which are more definite if not more convincing than the pathological evidence. Bernard,[2] more than twenty years ago, and Heidenhain [3] somewhat later, showed that, after section of the nerves of the salivary glands, a flow of saliva followed, which soon reached a maximum, and declined as regeneration of the nerves took place. This so-called "paralytic" flow of saliva is not an evidence that secretion is independent of nerve-influence, but rather of the view I am aiming at developing in this paper—that in a gland secretion is only a manifestation of the healthy life of its cells, an *essential* part of their nutritive history. When the nerves of a gland, after section, are not regenerated, such gland undergoes degenerative changes (Heidenhain). As is well known, the changes in the cells of the digestive glands dependent on nerve-stimulation can be observed

[1] Graham, of Toronto, reported, at a recent meeting of the Ontario Medical Association, a fatal case of herpes of the face, in which lesions had been traced microscopically in the ganglion of the nerves of the affected region.
[2] Robin's Journal de l'Anat. et de la Phys., i, p. 511.
[3] Breslau Studien.

microscopically, even in the living gland. As to whether there are separate [1] nerve-fibres concerned in the various processes which constitute the cycle of changes in gland-cells is, for my present purpose, a minor consideration. That there should be division of labor in nerve-fibres, as elsewhere, seems *a priori* probable. But it is important to have it clearly understood that the changes effected are directly dependent on nerves, and not necessarily on blood-pressure or the blood itself. The well-known fact that there may be secretion of saliva in a decapitated animal, on appropriate nerve-stimulation; and that on stimulation of the sciatic nerve in an animal already dead, as I have myself demonstrated in the cat, there may be sweating of the corresponding foot, shows clearly that while the blood supplies the crude pabulum, the real life of the tissue is absolutely dependent on nerve-influence; and be it noted that unless this *functional* life is maintained the tissue, as a tissue, whatever it be, gland, muscle, or what not, degenerates. Not to multiply instances, I think it will now be clear that the ground I have taken as to the constant outflow of nerve-influences on all the tissues being essential to their welfare and functional integrity is well supported by different kinds of facts. The reason that functional activity is bound up so closely with the *highest* welfare of a tissue is, that more of the vitalizing nerve-influence is called forth under such circumstances. I wish especially to insist that the functional activity of an organ is in reality but little more than an exaggeration of its resting state. This is shown by the chemical products of muscular metabolism being only increased, not different, whether the muscle be at rest or in action.

I do not forget that there are groups of animals in which no nervous elements of any kind have yet been recognized; but, in the first place, the nerve-horizon, like the palæontological, is being pushed lower and lower by our

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[1] Journal of Physiology, vol. vi.

improved methods and instruments ; and, again, such organisms are of the simplest kind, and their co-ordinations are few and uncomplicated. In mammals, and especially in man, the nervous system is the sovereign—a somewhat considerate one, however, ready to listen to the slightest prompting from any tissue however humble, but one whose subjects have so long been dependent on him that now they are absolutely unable to take care of themselves, or even subsist without him, however abundant the nutritive pabulum may be.

Granting this position to be sound, it is not difficult to understand why sexual excesses, with their enormous drain upon the nerve-forces, should leave a residue inadequate to meet the demands of the various tissues and organs. The heart is constantly in need of nerve-support ; and when that is not forthcoming, the various evidences of imperfect nutrition manifest themselves, such as irregularity, dilatation, etc. The cause of the "irritable" heart is thus patent. The sudden and marked alterations in blood-pressure, in the rate of the heart-beat, and other disturbances owing to nervous overflow, effects manifest, *e.g.*, in the respiratory system, through its own centre—are all causes, or rather occasions for the cardiac disturbance ; but, as I view the condition of things, quite subordinate to the general loss of nerve-force—the emptying of the reservoirs of life, as this view will warrant denominating the nerve-centres.

Testing the question of athletic strain from this standpoint, we proceed to ascertain how the facts harmonize with the theory. It is notorious that some men cannot successfully undergo training at all from the athletic point of view. They every now and then break down somewhere, if not generally ; they "go off training," as the expression is. The meaning of this is, simply, that the organization of these men is such that it cannot bear the nutritive strain demanded by the excessive muscular exercise ; and if nutritive capacity is dependent largely on the supply of central nerve-force, it is easy to under-

stand the condition. The break-down of athletes is often in the digestive organs. Force has been unduly diverted to the muscles, and insufficient is left for the other tissues. Consider what happens with an average college racing crew. Such men are supposed to do some study for at least a portion of the year; they do not understand their capabilities like old professionals; and, above all, the circumstances in which they are placed, with the eyes of a whole college, and, as they feel, perhaps, of an entire country upon them, they are the subjects of nervous exhaustion, owing to undue emotional excitement, for weeks before the contest. Often they sleep badly for days prior to the final event, and enter the boat conscious that they are not at their best. But they are determined to win or die; and with corresponding results in not a few cases.

I am persuaded that we are justified in assuming the existence of what I shall denominate *residual nerve-force.* On this, Nature does not usually call, even when a man makes powerful efforts; but, under extraordinary excitement, even this last resource is drawn upon, and invariably with the result of leaving the organism more or less bankrupted.[1] Such happens often, I believe, in the boat-races of amateurs. One of the most famous oarsmen in the world informed me that for two or three days before a race he exercised very little, slept more than usual, and in various respects allowed his organism to have its own way, so to speak. Being of an equable mind and a model physically, with especially fine circulatory organs, he never seriously broke down. He economized his forces, which gathered head before the excessive demand

[1] The effort that sacrifices the constitution may be a mental one. The following incident will illustrate this: A clergyman, when crossing the Atlantic, a disaster having overtaken the ship on which he was a passenger, found himself suddenly in the midst of panic-stricken fellow-travellers and crew. The officers had lost all control. This man, by superhuman efforts, restored order: but the task bankrupted his vital (nervous) forces, and he was never afterward capable of anything in which serious responsibility was involved. I relate this incident on the authority of a well-known clergyman of the same denomination as the unfortunate hero himself. In a minor degree I think we may see the principle often illustrated. For example, it throws much light on the evils of competitive examinations.

was made upon them. One thing is certain, a man cannot use his mind actively and be an athlete at the same time; which is plain enough, according to the views I am maintaining, but is not explicable, so far as I can see, on any other grounds.[1]

Physiologists, as a class, have paid little attention to comparison of results obtained from the study of different groups of animals (the morphologists have been wiser in their generation); while individual differences have, too generally, either been ignored or eliminated as the evidences of unsuccessful or undesirable experiments. In gaining accuracy, they have lost in breadth and depth of vision. Being convinced of the dangers surrounding, if not necessarily incident to, such a course, the comparative method has pervaded all that I have myself worked out in cardiac physiology. H. C. Wood,[2] also, has pointed out that in certain races of hunting dogs and in wild rabbits the vagi are capable of exercising unusual control over the heart, etc. He infers that a similar difference may exist in successful athletes. My own papers abound in recognitions of individual differences of every kind. Physiology must recognize such, if ever it is to explain facts fully for the physician.

There are some remarkable facts in connection with heart disease which have never been adequately explained, but which seem to follow almost as corollaries from my theory. Let us examine hypertrophy and dilatation in the light of these explanations. Hypertrophy occurs when the heart does increased work—when the intra-cardiac pressure is augmented. The obstruction leading to this is not the real or efficient cause of the hypertrophy; nor is the increased flow of blood to the

[1] I speak, in the matter of athletism, both from observation and experience. Some years since I did a good deal of hard rowing, though I never felt warranted in entering upon competitive trials. The only occasion on which I noticed marked irregularity of my own heart was on rowing in the evening of a day spent in close mental application. This experience conveyed a practical lesson to me upon which I have acted ever since.

[2] Quoted by Osler in Am. Syst. of Med., vol. iii., p. 632. Article: Dilatation of the Heart.

muscular tissue, during and after contraction, the real cause, any more than increased feeding is the cause of unusual growth in an organism as a whole. The growth depends on the assimilative capacity which resides in the tissue itself, and which is dependent on the vitalizing nerve-force (neuro-trophic influence) sent to it from some centre. How is it that there is failure of compensation in hypertrophy? How is it that such failure is sudden in some persons? How is it that under circumstances when the mechanical strain on the heart is approximately equally great for each person (racing crew), only one may suffer from any form of cardiac failure? How is it that dilatation may occur suddenly from some emotional strain, and be recovered from? These and many other apparent puzzles become clear enough if we suppose that the neuro-trophic influence, necessary for the maintenance of every tissue in health, is not forthcoming. I wish especially to insist that in this respect the heart is not exceptional. It falls under one broad law which applies to the whole vital economy. There are, of course, cases in which the failure—be it local softening, myo-carditis, or one of various other lesions—is clearly owing to deficient pabulum, the blood not passing freely into the coronary arteries. Such cases find a ready explanation.

It is pleasing to notice that we are beginning to realize that there may be serious disease apart from any changes in a tissue recognizable by the microscope. Medicine is emerging from the dominancy of anatomy, with its grosser conceptions. Niemeyer,[1] quoted by Osler,[2] who assents to his view, held "that it is not possible by means of the microscope to recognize all the alterations of the muscular fibrillæ which diminish the functional power of the heart." I have myself elsewhere[3] drawn attention to serious physical changes having been ex-

[1] Text-book of Medicine, vol. i., Am. ed.
[2] Am. Syst. of Med., vol. iii., p. 633.
[3] Journal of Comp. Med. and Surg., vol. viii., No. 2.

perimentally *demonstrated* in arteries, with at the same time no changes of their coats, even microscopically recognizable, and have suggested some important questions for consideration. If we but once thoroughly realize that the most pronounced functional changes of an organ are not necessarily connected with other than molecular changes, I am convinced we shall be in harmony with the best teachings of science, and be prepared for advances both scientific and practical. A good deal of interest has been manifested in mimic aneurisms, so-called. Osler[1] has very recently directed attention to a case in which there was dilatation of large arteries with no visible lesions. This also finds its explanation under the neuro-trophic theory which I advance. Such dilatations may be temporary (mimic aneurisms); they usually occur in nervous persons; and they are probably due to molecular changes in the vessel-wall, under the influence of the nervous system, in some way not yet clear as to details. That some molecular change takes place in the nervous centres themselves as a consequence of sexual excesses is evident, when we consider the impairment of mental power, and especially of memory, that follows, and which is gradually recovered from in favorable cases after a complete correction of the habits. The fact that the changes are molecular at first in many, if not all diseases, has encouraging aspects to it. What the nervous system has done under worry, etc., it may undo. I would point out, by the way, that the Weir Mitchell method of treatment *practically* recognizes the neuro-trophic theory; were it not so, the feeding part might do more harm than good. If my views are correct, it is possible to make exercise a remedy for cardiac and other diseases—in fact, such follows as a natural sequence to the theory. But, above all, the question must often be, "Canst thou minister to a mind diseased?" The very fact that such ministration

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[1] Canada Med. and Surg. Journal, July, 1887.

has been successful is also in itself an additional confirmation of what is now advanced. Flint [1] has well pointed out that in recruits there is often much mental disturbance; otherwise the merely mechanical strain on the heart would probably be, in many a case, quite insufficient to induce the "irritable heart."

It is a singular fact that in some maladies of the heart the left ventricle is more affected than the right. There are reasons for this beyond those that appear upon the surface, such as increased mechanical strain of this ventricle, which is, of course, from my point of view, only the occasion, not the real cause, which is increased nutritive strain. I submit some other facts for consideration. In the evolution of the heart in the animal scale we have a series of graded complexity: First, a simple pulsating tube, as in worms, gradually getting more complex, till, in the lower vertebrates (fishes), we have the *sinus venosus* and the single auricle and ventricle; in one group of fishes (dipnoi) a second less perfect auricle is found; in the batrachians two auricles, both well developed; in the turtle-tribe a partially divided single ventricle; while it is not till the alligator is reached that we have the five chambers separate and complete. After watching the hearts of the various cold blooded animals I have studied, through all their different phases of nutrition, I was able to determine the relative vitality of each part of the heart, and to reduce the results to the form of a general law,[2] viz.: That the part of the heart soonest to die, the conditions of nutrition being equal, is the ventricle—and of this the right moiety, when the chamber is not completely divided off; or, if there be two ventricles, the right has the greatest vitality. Invariably the *sinus venosus* is the last to pulsate. The hearts of the higher mammals also conform to this law. I have seen the great veins at their junction with the heart (representing in

[1] Am. Syst. of Med., vol. iii., p. 752.

[2] The Rhythm and Innervation of the Heart of the Sea-turtle, Journal of Anatomy and Physiology, vol. xxi.

part the *sinus venosus*) pulsating, in a rabbit, long after the whole of the rest of the heart had ceased to beat. The reason of the greater vitality in mammals of the parts indicated is clear only on the principles of evolution. It is scarcely a metaphor to say that man carries in his bosom something of the fish, tortoise, snake, etc., and well for him he does, otherwise his years would probably be fewer than threescore-and-ten. The left ventricle is a late and special development to meet the needs of an exceedingly complex organization. It must be highly sensitive, to respond to the countless adjustments required of it; but that very sensitiveness exposes it to disease.

It is not possible to understand in a broad sense the relations of cardiac diseases in man, without bearing in mind the enormous and disproportionate development of his cerebral lobes. The cerebrum is constantly interfering with the action of lower centres and modifying the whole life of the animal. Witness reflex fainting by vagus-inhibition effected through afferent nerves from the eye, the ear, etc. (appalling sights, evil tidings); but the countless inhibitions or other interferences, less striking, pass unnoticed by us, though not unfelt in the heart's nutrition. Man is essentially a worrying animal; fortunately he is also the most susceptible to emotions that are replete with health-restoring energy, which can be readily understood in the light of my neuro-trophic theory. In this connection the cases recently alluded to by Broadbent[1] in his Croonian lectures are most suggestive, and are confirmative of these views.

I wish to bring to the notice of philosophic physicians certain observations and experiments which deserve to be better known. Kronecker has drawn attention to a certain peculiar and (in the dog) fatal behavior of the heart, which consists in an unco ordinated fibrillar action of the ventricles, wholly beyond vagus-control. It may be induced by needle-puncture, electricity (and when

[1] British Medical Journal, March and April, 1887.

the heart is much exhausted, even by manipulation), applied to a certain region (regions?) of the heart. Kronecker thought it due to injury of a hypothetical co-ordinating centre; but from my own study of the hearts of the cold-blooded animals, as well as from what I have seen of it in the dog, I am inclined to attribute the phenomenon to the independent action of the muscular fibres themselves when their nutrition is bad; or, at all events, not through any action of ganglion-cells, though possibly the nerves of the cardiac substance may be concerned. Some stimulus is, of course, required—often but a slight one, to induce the result. Kronecker thought this behavior of the heart explained why very slight wounds of this organ sometimes proved fatal; while at other times larger ones seemed of little consequence.[1] I would suggest that this peculiar action of the heart in lower mammals, etc., throws much light on the behavior of the heart of man in certain ill-understood conditions, as in angina, especially when fatal, and, perhaps, in thrombosis of the coronary arteries; indeed, in any condition in which there is defective nutrition of a well-pronounced character. I have elsewhere[2] called attention to certain experiments on ligating the coronary artery which deserve special mention in connection with the pathology of those affections referred to above. Flint[3] and others have argued well for ischæmia as a cause of angina; and the experiments just referred to, as well as Kronecker's and my own, lend strong additional support to such a view. I would, however, substitute *mal-nutrition*, of which ischæmia may itself be the chief cause, as a conception of the cardiac condition more in harmony with the most recent physiological views. The nervous element must not be left out, for certain attacks of angina (as in Hunter's case) are directly trace-

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[1] The writer has given an account of this phenomenon in the Medical News for July 9, 1887.
[2] Canada Med. and Surg. Journal, February, 1887.
[3] Am. Syst. of Med., vol. iii., p. 758.

able to emotion. The possible effects of vasomotor nerves must also be taken into the account, and largely, too.

Before passing on to the second part of this paper, I may remark that there are many points either referred to or developed in the recent lectures of Macalister, and also of Warner,[1] on allied subjects, which tend to strengthen the theory of nutrition I am maintaining in this paper, but to which the limits of space will not permit me to refer.

II.

It now remains to indicate: (1) Which nerves constitute the channels by which nervous forces reach the heart and affect that organ; (2) what the manifestations of such influence really are. It is especially on this part of the subject that my own and others' researches on the cardiac nerves have thrown a new light. It will be necessary, however, to deal with this portion of the work much more briefly than, perhaps, might be desirable. But those who are inclined to look more carefully into the matter will probably be willing to consult the original papers.[2]

The depressing action of the vagus nerve was that chiefly regarded by physiologists, till very lately. As will be seen, this depressing action is but a part of the whole, and the less important part by far. After the most careful examination of the action of the pneumogastric nerve on the heart in the water-tortoise[3] (Slider Terrapin), under every conceivable condition of cardiac

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[1] British Med. Journal, 1887.

[2] My own researches on the cardiac physiology of the Sea-Turtle (first paper), the Slider Terrapin, the Fish, and Menobranchus, have appeared in the Journal of Physiology, vols. v., vi., vii., and on the Alligator, the Sea-Turtle (longer paper), in the Journal of Anatomy and Physiology, vols. xx., xxi. My paper on the heart of the snake is to appear in the October number of the Journal of Anat. and Phys. for the current year.

Most of the papers by others on the innervation of the heart will be found in the Journal of Phys. A paper on the heart of the frog by the German physiologist Heidenhain appeared in Pflüger's Archiv, bd. xxvii.

[3] Journal of Phys., vol. vi.

nutrition, I was led to conclude that this nerve was essentially bound up with the best welfare—the highest nutritive interests—of the heart. I found, further, that a comparison of hundreds of experiments justified the law I then laid down, viz.: *That the beneficent action of the vagus was in direct proportion to the needs of the organ at the time.* When I discovered this connection, which all my own subsequent investigations, as well as those of others, have only tended to strengthen, I felt that there must be a world of physiological meaning in it, and a significance of the highest moment for medicine. Instead of dwelling on multitudinous details, I shall refer to a single experiment which speaks volumes in itself. In a tortoise that had been the subject of experiment during two successive days, the sinus and auricles were beating but slowly and feebly ; the *ventricle, with the exception of a few fibres, had ceased.* After a series of stimulations of the vagus, the rhythm was more than doubled, and the ventricle, which at first responded only to every two or three beats of the auricles, *then pulsated after each beat* of the auricles. It will be noted that similar though less marked good effects have been witnessed as a result of vagus-stimulation in all the animals examined by me ; and I have seen no exception to this, though I must have stimulated the vagus thousands of times during the past three years. But the sympathetic accelerator fibres and the sympathetic main stem have appeared in a new light also. The sympathetic cardiac accelerators have now been pretty thoroughly worked out both anatomically and physiologically, by others and myself, for several cold blooded animals.[1] When working on this subject in the terrapin certain remarkable, puzzling, and hitherto unobserved effects were noticed. Thus, not only did stimulation of the part of the main sympathetic cord (in which the accelerator fibres run

[1] See the various papers previously alluded to in the foot-note on page 25, especially those in the Journal of Phys., vols. v. and vi., and Journal of Anat. and Phys., vol. xxi.

upward to the ganglia, from which they finally proceed as isolated branches to the heart) produce augmentation of the force and acceleration of the rate of the beat ; but as I pointed out, for the first time, this primary effect was followed by slowing and irregularity of the heart. It was thus seen that the vagus and sympathetic had a physiological resemblance ; and what I wrote then, " The vagus is a sympathetic with inhibitory fibres, the sympathetic a vagus without them," has proved true. I had noticed [1] that the immediate effects of stimulating that portion of the main sympathetic stem in which the cardiac accelerating fibres run had, in one instance, produced effects as remarkable as those cited above for the vagus. It was observed that both nerves had the power of augmenting the heart's activity ; with the vagus it was an after-effect, with the sympathetic a primary one. In the case of the vagus the slowing, irregularity, or arrest is primary ; with the sympathetic it is secondary. The nerves were thus seen to be physiologically similar, and yet different. Gaskell,[2] by stimulation of the sympathetic fibres, which run in the vagus trunk, while still separate within the cranium, in the frog, got purely depressing effects ; and I found irregularity and other evidence of a depressing influence on similar intra-cranial stimulation in the terrapin.[3] The evidence was thus accumulating to give a substantial basis to Ransom's [4] conception that there might prove to be in the vagus true inhibitory fibres which tend to increase *constructive* cardiac metabolism, and sympathetic fibres which favor *destructive* metabolism. Gaskell[5] has, in a more recent paper, further elaborated this theory ; so that now we may feel warranted in regarding the vagus as being demonstrably a vago-sympathetic nerve. It therefore follows that those influences of a nutritive character which reach the heart

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[1] Op. cit., p. 268.
[2] Proceedings of the B it. Phys. Soc., Journal of Phys., vol. v.
[3] Op. cit., pp. 275-277.
[4] Jou nal of Phys., vol. v.
[5] Ibid., vol. vii.

pass by three different avenues: (1) As conservers, constructors of cell-tissue, by the vagus in its inhibitory fibres proper. (2) With the reverse action, by sympathetic fibres in the vagus itself and in the main sympathetic stem, finally to emerge as accelerator (augmentor) nerves. When the vago-sympathetic nerve is stimulated, the inhibitory fibres have apparently the predominant effect; that they continue to do so to the very end of the chapter I am, however, by no means certain. It is true, these conclusions are founded chiefly on the study of cold-blooded animals; but it is likely that so general a law holds in the main for mammals, including man. In making the applications to pathology it may, first of all, be remarked that it is within every observer's experience that events may take place in the cerebrum which inhibit or otherwise influence lower centres. Witness the cessation of labor-pains on the entrance of the accoucheur; the behavior of the sexual centre; the inhibition of the defecating centre by mental preoccupation at the usual period of its action.

Hewan,[1] reporting his own case, states that after intense study his heart-beat fell from 72 to 28 in the minute. It is possible to interpret this result either by the increased action of the inhibitory fibres or deficient influence from the sympathetic fibres. At all events, there was cardiac disturbance, plainly traceable to the nerve-centres.

What is the mechanism of cardiac acceleration? Usually Marey's law holds that "the rate of the beat is in inverse ratio to the arterial pressure." In disease, however, the heart has often to choose between two alternatives, either to contract against undue resistance or not to contract at all. Normally, as Sewall[2] has shown, the depressor nerve (rabbit) acts as a reporter to the cardio-inhibitory centre, indicating the degree of intra-cardiac blood-pressure, and thus the heart in health conforms to

[1] London Med. Times and Gazette, March, 1875.
[2] Journal of Physiology, vol. vi.

Marey's law. In a case of dilatation, following, say, an athletic contest, it is possible to understand that the destructive metabolism, brought about by the accelerating sympathetic fibres, was so far in excess of the constructive, effected through the vagus inhibitory fibres, that molecular changes take place which are the real cause of the dilatation. The latter, as I view it, implies essentially molecular changes in the cardiac tissue. We must not conceive of the heart as of so much leather stretching under obstruction. When a skeletal muscle acts against a certain opposing force it does more work; but when that force is increased beyond a limited point the muscular elasticity is lost for a time. This can be shown experimentally on excised muscles. The loss of elasticity indicates molecular changes; but the case of the heart is not precisely parallel with that of an excised muscle. It has the choice of being stretched or not, so to speak, and it does not elect the former *so long as the balance of nerve-force is maintained.* This balance is preserved or lost according as the influences which reach it through the inhibitory fibres of the vagus exceed or fall short of those by the sympathetic.

The present state of cardiac physiology enables us the better to understand such forms of disease as Basedow's; how, for example, the mental state can influence the heart for good or ill. Many German writers,[1] Bartholow[2] and others, have stated that galvanization of the nerves (sympathetic) has proved valuable in the treatment of Basedow's disease. It is well known that the conductivity and irritability of a nerve may be lowered by the passage of a constant current through it. As this effect is temporary, we may suppose it due to some molecular change; but from this result we can understand the good that follows the use of electricity, whether the undue irritability in this and other diseases be referred to the muscle, the nerve, or the discharging centre.

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[1] Am. Syst. of Med., vol. iii., p. 767.
[2] Medical Electricity.

Perhaps the time is not yet ripe for us to determine in a special case whether the vagus-fibres proper or the sympathetic fibres are at fault ; but it must be clear that we now know enough to enable us to adopt theories less mechanical, more fundamental, and more in harmony with the ascertained facts, not only of medicine but of other sciences. In connection with these considerations, what has been insisted upon in the earlier part of the paper must not be forgotten. After all, the condition of the nervous *centre* is the main thing in most cases. It is not to be forgotten that nerves are but conductors, which may, however, be diseased. I think I can myself foresee how such views as I have endeavored to present in this paper must lead to a wiser prognosis and a more rational treatment.

It seems desirable that we should endeavor to form some definite conception of why and how the heart happens to beat at all. This problem I attempted to solve, as best the evidence we now have would allow, in a paper[1] published a few months since ; and I present here for consideration the summarized conclusions of that study.

1. The factors entering into the causation of the heart-beat of all vertebrates as yet examined are : (*a*) a tendency to spontaneous contraction of the muscle cells composing the heart ; (*b*) intra-cardiac pressure ; (*c*) condition of nutrition as determined directly by the blood, and indirectly by the nervous supply of the organ.

2. The tendency to spontaneous contraction of muscle-cells is most marked in the oldest parts of the heart ancestrally considered. I have shown (*Jour. of Anat. and Phys.*, vol. xxi.) that in the sea-turtle the last segments of the ventricle to pulsate are on its extreme right ; while the right auricle outlasts the left, and the sinus and great veins beat much longer still. The same has been noticed

[1] Causation of the Heart-beat, etc., Canada Med. and Surg. Journal, January, 1887.

in several other groups of animals. The most recently acquired parts of the heart always are the first to lose functional activity. These are but extensions of Harvey's observations, seen in the light of evolution.

3. In all hearts examined, intra-cardiac pressure is a factor of considerable importance; in some, as that of the Fish, Menobranchus, etc., it is apparently the controlling factor. The same may be said of the molluscan heart.

4. The power one contracting cell when in action seems to possess of initiating a similar state in others, is of great significance.

5. The influence of the nerves of the heart appear more and more as we ascend the animal scale. They seem only indirectly concerned in the causation of the beat by their influence over nutritive processes; but as the heart is being so frequently modified in its action, their influence in highly developed hearts becomes an almost constant factor, and of a degree of importance which our knowledge of the relation of nerve to muscle enables us but inadequately to appreciate, but which the pathological changes ensuing on nerve-section illustrate.

6. It almost follows from the above that one part of the heart having contracted, the other parts must follow. This is probably the explanation of the rapid onset of the ventricular after the auricular systole in the mammal. It will be remembered, too, that even in the mammal contraction begins in the great veins entering the heart.

The basis of all these explanations is found, in reality, in the *natural contractility of protoplasm.* A heart in its most developed condition still retains, so to speak, the inherited but modified Amœba in its every cell.